

A Reply to Criticisms on the "Trichopterygia Illustrata," by
the Rev. A. MATTHEWS, M.A.

Since the publication of the "*Trichopterygia Illustrata*" various criticisms on that work have appeared in the periodicals devoted to Natural History; these I have hitherto refrained from noticing, in order to make my reply more comprehensive than it could have been at any earlier period. Many of the remarks contained in these critical notices have been very complimentary, and it is gratifying to find that the faults pointed out apply entirely to errors in the letter-press, pseudo-latinisms, objections to the style of the figures, or differences of opinion as to the comparative merits of certain authors whose works I have reviewed; while the scientific and only important portions of my work have been unassailed, and by letters which I have received from many of the most distinguished Entomologists of the present time, as well as by the published remarks of others, appear to have met with universal approval.

With regard to the first of those faults which I have named, viz, errors in the letter-press, which I regret to say are far too numerous, I feel that I must take the whole blame upon myself. I foolishly consented to revise the sheets as they were struck off, a task which their author should never undertake, since his perfect knowledge of the idea intended to be expressed and his capability of repeating almost every line by heart engender a carelessness, however unintentional, to which a stranger would not be liable, and of which I now suffer the evil consequences.

As to what have been termed my "pseudo-latinisms," these were in many instances perfectly intentional, and seem to me perfectly justifiable, *e.g.*, "in *Insula Hyeres*," or "*ad ripas fluvii Seine*" must be more intelligible to most readers than resuscitating such names as "*Stoechades*," "*Sequana*," and such like,

terms for many centuries out of use, for the purpose, as it would certainly appear, of exhibiting one's superior knowledge of ancient geography.

With respect to the plates no fault has been found with those which exhibit the genera, but a little more care on the part of the engraver would have greatly improved both the appearance and utility of those devoted to the species. This may very easily be proved by comparing Plates Nos. 29 and 30 with any of those which preceed them, in many of which the superficial sculpture has been very inadequately expressed, and requires a careful comparison with the description of the species.

But it is to the class of criticisms which assail my observations upon previous authors that I wish to direct attention. None of my remarks were made without careful consideration, and as they are supported by clear and obvious proofs, I trust to be able to establish the truth of all that I have written. Of criticisms of this class that published in the "*Stettin Ent. Zeitung*, XXXIV, p. 398, from the pen of Dr. Dohrn, is the most important and the most detailed. I will therefore examine his remarks seriatim. Referring to my review of Gillmeister's "*Trichopterygia*," Dr. Dohrn first of all quotes a notice of that same work published in the "*Stettin Ent. Zeitung*, T. VII, p. 59, (1846), in which Dr. Schaum says "the descriptions are splendid, short and to the point, they every where put proper stress upon the specific differences. But the most perfect in this classical work are the Plates, drawn by the author and engraved by Sturm; the most perfect specimens which the Entomological Iconographie has to show." Dr. Dohrn then quotes my own observations on Gillmeister (vid. *Trichopterygia Illustrata*, Introduction p. xii), and subsequently remarks "that it is more than curious that Schaum should call a work "splendid, perfect, and classical," of which Matthews' says that he only made the confusion worse confounded, added nothing but what was known, ignored the work of other Entomologists and misused their liberality." But to say that Dr. Schaum designated Gillmeister's work as "splendid, perfect, and classical," is to affix a meaning to the learned Doctor's words which they clearly do not possess; the terms splendid and perfect express merely Dr. Schaum's opinion of the descriptions and plates. Whether Dr.

Schaum was qualified to judge of the descriptions or diagnostic characters of a Trichopteryx may be gathered from his own words, words spoken at the time when he presented to me the collection of his American Trichopterygia. I had asked his assistance in determining their species, and in reply he said "I must leave them entirely to you, *for I know nothing whatever of that class.*" These were the very words of my esteemed friend, and uttered on such an occasion were indelibly fixed on my mind.

But I have never found fault with Gillmeister's specific descriptions, as far as they go they are correct and good; no amount of excellence in specific descriptions can however in any way atone for a reckless disregard of the commonest rules of nomenclature. Dr. Gillmeister was well aware that M. Allibert had already published descriptions of a great majority of the very species on which he was himself engaged, and he must also have known that M. Allibert's names had been accepted by the Entomologists of France, at all times an important part of the scientific world; if he considered M. Allibert's descriptions insufficient, he might, as I did, without difficulty have obtained an examination of the type specimens, but he adopted the readiest method of avoiding trouble, ignored the work of M. Allibert, and either substituted fresh names for his species, or quoted their names as synonyms of species to which they did not refer.

The manner in which he treated Dr. Aubé was equally inexcusable. Dr. Aubé with his usual kindness had forwarded type-specimens of all the species contained in his fine collection to assist Dr. Gillmeister in his work; they were subsequently returned to Dr. Aubé in a very unsatisfactory condition; to such species as Dr. Gillmeister had recognized he had attached labels, those which he did not know he had left unnoticed. It may be thought that I am making a bold assertion, but if the collection of Dr. Aubé still exists, anyone who will examine it may convince himself of the fact. For in that collection he will find two examples of one of the most distinct and most striking species of the whole family, viz, *Smicrus filicornis*, labelled as having been sent to Dr. Gillmeister, and returned without remark. There may be others found in a similar condition, though, as it is a long time since I have seen the collection, I

cannot now remember their names ; but one such fact is of itself sufficient to prove an amount of carelessness perhaps without parallel, certainly without excuse.

But, bad as this was, his treatment of Col. Motschulsky was far worse. In a paper published in the Bull. Mosc., 1845, V. II., p. 504, entitled "Ueber die Ptiliens Russland," Col. Motschulsky makes the following remark, "I said in the Stettin Zeitung that I had 33 species of Ptilium, Gillmeister wrote for them, received them, and returned them saying 'none new'" By the above date it is clear that the transaction alluded to must have taken place previously to the publication of Dr. Gillmeister's Monograph, it therefore both justifies my assertion that Dr. Gillmeister had seen Col. Motschulsky's types, and renders the publication of synonymy such as that given under the head of "*T. depressa*," (quoted by me at length in p. xii of the Introduction to the Trichopterygia Illustrata), utterly confusing and utterly inexcusable. But that example of synonymy is only one of many of a similar character, by means of which the confusion becomes disseminated throughout the work.

If Dr. Gillmeister had not seen Col. Motschulsky's types and could not comprehend his descriptions, which it is often scarcely possible to do, he should have omitted their names altogether from the list, or have classed them separately as "*Species incerte*," rather than have assigned them ad libitum to species with which they have no connection.

In addition to all these instances of nomenclature wilfully confused, I might reasonably have asked on what grounds did Dr. Gillmeister ignore the *Derm. pilosellus*, *brunneus*, and *nitidulus* of Marsham, or the *Scaphidium punctatum* of Gyllenhal, a name which had even then been recognized for about 40 years, and in the place of this last substitute the far less expressive term of *Trich. alutacea*? But I thought that my case had been sufficiently proved.

Dr. Dohrn also complains that I assert that Dr. Gillmeister in his Monograph did not bring forward a single fact which was not previously known, except his observations on the Metamorphosis ; and to controvert my assertion cites my having adopted four new species retaining the names which he had given them.

These Dr. Dohrn terms four new facts; he does not observe that my assertion referred to life-history and anatomy, and appears also to forget that two of these four species had been previously described by Dr. Erichson, to whom according to the strict law of priority I might therefore have assigned them. But knowing the unhandsome manner in which Dr. Erichson had abused the confidence of Gillmeister and had unkindly anticipated his work, I preferred to attach the name of “*Gillmeister*” to the species in question. If I committed a fault, it arose from my desire to do justice to Gillmeister.

When to the confusion of nomenclature pervading the whole Monograph is added the great amount of erroneous anatomy, the conspicuous Labial Palpi utterly ignored, a mutilated portion of the Stipes figured as the true Mandible, and false delineations given of the Mentum and adjacent organs of the lower part of the mouth; and forms so totally diverse as the species of *Ptinella* and *Ptenidium* comprehended under one and the same generic appellation; I think that I was fully justified in saying that such work left the Trichopterygia in a state of confusion far worse than that in which it found them.

In the case of Col. Motschulsky, my first impressions experienced a total revulsion. I commenced work a disciple of the common belief in his universal inaccuracy; I had imbibed the idea that his species were mere varieties, separated on untenable characters. Gradually step by step as my own knowledge of these insects increased, so pari passu did my conviction that Col. Motschulsky was right in his views. To assist in arriving at the truth, I determined to communicate at once with him and obtain authentic types of his species. These in large numbers he immediately forwarded to me with the same ready kindness as he had formerly shown to Gillmeister, but with a very different result. I found that though his descriptions were very obscure, his types were in most cases sufficient to convince any one of their specific value. Indeed if an experienced Entomologist has for many years turned his attention to the study of any special class of insects, he may reasonably be supposed to know more of that class than others who have comparatively neglected them. I soon found this to be true of Col. Motschulsky, and

have many times been compelled to accept his species in contradiction to my preconceived opinion.

Dr. Dohrn asks whether I expect him to accept Motschulsky's "99 species" on my authority? If he will refer to my work he will find that I expect nothing of the sort, I do expect that those species which I have verified by the careful examination of types will be accepted, and am fully prepared to support my position, and Dr. Dohrn must remember that their acceptance will not wholly depend upon his opinion.

But let us examine this matter more closely. The actual number of Motschulsky's *newly-named* species amounts to 85, and of these only 25 appear in my list of recognized species. Of the 43 supposed new species which I have verified by actual examination, 2 only appeared to be varieties, while 41 were good and distinct species; of these last, 16 had been previously described by other authors, and 25 were wholly new to science. Upon such facts it is only fair to conclude that a large majority of the other 42 which he has described would probably be true species. The rest of his names are merely manuscript without definition.

In the year 1845 the combined efforts of all other Entomologists had resulted in the discovery of 37 species of *Trichopterygia*, while at that same period Col. Motschulsky was acquainted with more than twice that number, of which above one-half were the fruits of his own persevering exertions in their pursuit and superior knowledge of their habits. Well and truly then might I say that "his knowledge of the *Trichopterygia* exceeded that of any other Entomologist."

To contradict this assertion Dr. Dohrn brings forward the fact that Motschulsky had at one time considered the *Trichopterygia* to possess pentamerous tarsi. I need not again recount mistakes of a character quite as important made by professed anatomists to prove that the wisest among men are liable to error, others have committed the very same mistake, and such indeed was at one time my own impression. The tarsi of a *Trichopteryx* are surrounded by very long diaphanous setæ, which in mounted specimens will occasionally cross the long terminal joint of the tarsus in an oblique and so perfectly natural

direction, that any one might suppose the terminal portion to be an amalgamation of three parts, nor would be convinced of the illusion without a careful examination of tarsi obtained from other specimens. But be this as it may, it cannot materially affect my expressed opinion of Col. Motschulsky. I never said that he was an expert anatomist, though I might have thought that his knowledge of the subject was quite equal to that of his contemporaries. I have said before that I am not qualified to express an opinion of Col. Motschulsky's proficiency in other sections of Coleoptera. I have said before, and I say it again, that his knowledge of the *Trichopterygia* exceeded that of all other Entomologists.

A little farther on in his Review, Dr. Dohrn remarks that the name “*Dohrnii*,” given by me to the *Trich. fuscipennis* of Haldeman, cannot stand, because “by the author's own opinion *Trich. fuscipennis* is a synonym, and as there is no like-named species except the *Ptilium fuscipenne*, which dates from 1849, and is thought by the author synonymic with *Pt. Spencii*. Also Motschulsky's species *Acratrichis brunnipennis*, (Amer), Matthews remarks “An *T. Dohrnii* par.” The facts which Dr. Dohrn has thus cited appear to me in themselves sufficient to quash the name “*fuscipennis*” altogether. It is evident that Professor Förster's *Ptilium fuscipenne* was anterior in date to *T. fuscipennis* of Haldeman, it is also certain that the same name had been applied to another species by Gillmeister, and recorded in p. 48 of his “*Trichopterygia*.” If I had been able to recognize that species I must of course have retained its name. But though I failed, some other author may succeed in doing so, and its name may now be restored to the list without creating confusion.

It appears to me that the only safe basis of nomenclature is to adhere closely to this rule, *i.e.*, that when once a name has been used to designate a certain insect, the same name cannot subsequently be applied to any other species of the same section. I say section because genera are in their very essence changeable terms, wholly subject to the will of each separate author. If therefore a specific name should merely be restricted to a genus and might again be repeated in the genus next in succession, endless changes of nomenclature would ensue, especially in a

section like this, of which the whole species have already been comprised by more than one author under the name of "*Trichopteryx*," and by others under the name of "*Ptilium*."

Taking the case before us as an example, let us suppose that the name *T. fuscipennis* is restored either to Gillmeister's or to Haldeman's species, and also that the *Ptilium fuscipenne* of Förster, and the *Ptenidium fuscipenne* of Motschulsky are both eventually discovered to be true species, the name, if not restricted to a single genus, must be repeated in all the three. Subsequently some author averse to sub-divisions re-unites the whole section under one generic term, two recognized names must then be changed for fresh appellations; but as soon as these new names had been accepted, another author arises, who determines to divide the section into various genera once more, and in consequence must again alter the names last given; and so on ad infinitum till at length the term "*fuscipennis*" would convey no possible idea either of form or species. By adhering to the rule I have mentioned, and on which I have acted, all such confusion will be avoided, while by pursuing the contrary plan endless and most harassing complications might be caused.

In the next place Dr. Dohrn mentions errors which he has observed in my account of the capture of certain species, errors so utterly frivolous that the very notice of them serves to show the difficulty of finding faults. He says "that Dr. Schaum never was in Brazil, therefore the notes on *Trich. Wenkeri* and *discolor* are incorrect is certain. The like is the case with the Californian *Pt. pullum*, which Dr. Mäklin has certainly not caught in loco." At the interview between Dr. Schaum and myself previously alluded to, he pointed out a certain group of specimens, all ticketed with his accustomed care, and said, "you must remember that this lot were taken in Brazil, all the rest were found in the United States;" and, as he said nothing to the contrary, I supposed that he meant they had been taken by himself. At any rate, if *T. Wenkeri* and *discolor* were actually taken in Brazil, the name of their captor must be of secondary importance in geographical relations. Whether Col. Motschulsky was ever in Ceylon, or Dr. Mäklin in California, must be equally unimportant. I can only refer the reader to my extracts from previous authors, and add that my type of *Pten. pullum*

was received from M. le Comte de Mniszech, ticketed thus, "*California Mäklin.*"

In the last part of his Review, Dr. Dohrn would seem to imply that I made use of Latin in order to exhibit my superior education; the truth is that I made use of Latin, especially in the original descriptions transcribed from other authors, in order to assist persons *as ignorant as myself*, those who cannot read the languages of Germany, Sweden, or Russia. Had I used entirely my native tongue the difficulty would have been increased, as still fewer could have read English. By making use of a medium of communication universally recognized, I hoped to render my meaning intelligible to all. In reference to the mistakes which I have made, I can only hope that anyone who has read the Latin in the pages devoted to the anatomy of the *Trichopterygia*, would hardly suppose that such an outrageous blunder as "in paludibus Comitis Cantabridgiensibus," (triumphantly paraded by Dr. Dohrn), could have proceeded from ignorance of the language. Its true history is this, I had originally written "in paludibus Comitis Cantabridgiensis," and subsequently altered it thus, "in paludibus Cantabridgiensibus," the obliteration was overlooked by the printer, and *although corrected a second time* in the proof-sheet, the error was again with extreme carelessness reproduced in the final impression in all its deformity, a fact unnoticed by me until too late to rectify the mistake. "Tauria" is quoted from Motschulsky, and left unaltered because I did not know for certain whether he intended it for the Crimea. The transposition of the vowels in "Mадiera" arose from my own carelessness in writing the word originally, and in overlooking the error in the proof. This name is spelled correctly in my extracts from Mr. Wollaston.

I think that I have now noticed all the criticisms of Dr. Dohrn. The last few lines of his Review express, I would fain believe, the real feelings of their writer more truly than his previous remarks. I am not surprised that Dr. Dohrn should have taken up arms in defence of his countryman, I respect and honour his spirit, though I doubt the expediency of eliciting more positive proofs of his friend's misdoings. I have endeavoured on my own behalf to support my position, and if in so doing I have anywhere exhibited unnecessary asperity, I fear the

pages of the Review must have conveyed the infection. I can never forget the kind manner in which Dr. Dohrn assisted me in my work, and however we may differ in our opinion of other men, I trust our regard for each other may remain unchanged.

To Reviewers, who in a work which has carried the oral anatomy of Coleoptera to heights never before attained, and has revealed functions of certain organs previously unthought of, can find nothing more worthy of notice than errors in the letter-press and supposed defects in the plates, I need not reply; their own remarks prove the spirit by which they were inspired, and supply an answer to themselves.

As I do not intend again to revert to this subject, I will add a few words on the anatomy exhibited in the pages and plates of the "*Trichopterygia Illustrata.*" In the Appendix I have given a detailed list of the vast number of preparations which I made, in order that by comparing them with each other I might obtain a correct idea of the true outline of each separate organ. These preparations have been minutely examined and compared with my figures by Drs. Le Conte, Horn, and Sharp, and by Messrs. Crotch, Wollaston, and Janson, who all concurred in affirming the accuracy of my delineations. If anyone should still feel sceptical on that point, my preparations are open to his inspection, and can be selected by the numbers affixed to them in the list. They are preserved in Canada Balsam, which up to the present moment has retained its pristine transparency.